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Genesis of the Animikie Iron Range

—BY—

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A Paper read at the Annual Meetings of the Institute,
Toronto, 2nd, 3rd and 4th March, 1904

Published by Authority of Council
At the Office of the Secretary, Orme Building
Ottawa, 1904

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PREFATORY NOTES.

When we contemplate writing upon some certain subject, which has also been dealt with by a number of other writers, it is absolutely necessary to read the principal publications, at least, in order to see whether we may not be duplicating what is already a matter of common knowledge, or whether there may not be something previously written on the same subject which might to some extent prejudice our efforts.

Accordingly, before commencing work upon this paper, I came across certain notes in one monograph, which, if overlooked and not rectified by me, might have caused some of my own statements to seem peculiar. I must therefore ask the indulgence of my readers if I have to devote considerable space to the refutation of the statements of this monograph, even though it may place the principal subject of this paper in a more subordinate position.

When we take into consideration the immense field which would have to engage our personal attention and study, it may well be imagined that it is next to impossible to become thoroughly versed in the geological conditions of every locality from personal examination, and we therefore are compelled to rely oftentimes on the published observations of others or perhaps to some extent on hearsay information. Thus it will sometimes happen that we may make statements which, after personal examination, we would never have made, though the danger of hostile criticism may be averted by quoting the source of our information, but if, instead of this, we come forward and make positive assertions then we must expect to be controverted.

The discoveries of the Lake Superior iron ranges in Minnesota, Wisconsin and Michigan were usually the result of a fortunate combination of circumstances, but, when once these were established, it was comparatively an easy task for one who was well acquainted with them to determine that the Animikie rock series north of the interna-

tional boundary was indetical with the formation on the above-mentioned ranges. Indeed, the rocks in which iron ore occurs were readily recognized, but it was a more difficult matter to determine whether iron deposits did actually exist in them. It required a long and tedious study of the geological, petrographical and chemical conditions which have been active here, and these had to be viewed in the light of experience gained on the other ranges. Having done this, and treating the different indications and traces found here as part of a complete system, I finally was forced to conclude that iron ore exists on the Ontario side of the line under the same conditions as in the Mesaba and other ranges. These researches and the minute examination of rocks, ores, diamond-drill cores, and rock sections, made me at last so familiar with the formation that I am confident of now being able to give close and accurate descriptions of the appearance of the minerals in the field, and present a reasonable account as to what their origin must have been.

In my treatment of the genesis of the iron ore deposits, I have departed from the well beaten path, over which for long years a number of chemical reactions and physical phenomena have been carried, and I have taken another route, which may appear too rugged and difficult for many to follow me, but I leave it to time to make the way more smooth, and trust that, with here and there an improvement, I may have pointed out the road to a clearer recognition of the forces that went to the formation of our iron ore deposits.

I devote considerable space to the geology of this region, a course which seems to me indispensable, not only because of the somewhat different classification of the series by Canadian and United States geologists, but also because it will tend to make more readily intelligible some of my hypotheses as to the origin of certain of the rocks of which I treat in this paper.

A NEW IRON RANGE.

Notwithstanding the fact that the bibliography on the iron ore deposits in the vicinity of Lake Superior is quite voluminous, a new contribution to this literature, treating of a newly discovered territory

or iron range, should prove of considerable interest to those who, from a scientific or a commercial standpoint, are concerned in the finding and development of new and large bodies of high-grade iron ore, including not only the iron masters, but also geologists and mining engineers. It is perhaps hardly necessary to emphasize why these should be interested, for not only will there be in all probability fresh problems in geology to be solved, new mines to be opened up and more deposits to be revealed, but also we all know that of the merchantable iron ore at present known, at least 99 p.c. is in the hands of one company, upon which thus depend all other concerns interested in the smelting or selling of this mineral. We know also that the quantity of ore of Bessemer quality in sight is limited, considering the proportions which the iron industry, on the northern portion of this continent alone, has attained during the last few years. Furthermore we know that we are already mining and using ores of much lower grade than the Bessemer limit formerly demanded, and the use of these will grow in proportion as the supply of the higher grades still existing diminishes in quantity, and prices are raised. There will come a time when iron production will become more expensive through the adoption of different methods of smelting and reducing these ores, and those companies who are fortunate enough to be in possession of large bodies of high grade ores will have a vast advantage over those who are compelled to change their present mode of working. To all concerned, therefore, it would be an inestimable boon if there should be discovered another iron range near the shore of Lake Superior of as great an extent, if not greater than the Mesaba or any other Lake Superior iron range.

The new range alluded to has, so far as it has been examined, a length of over 25 miles, with an average width of about 6 miles, and lies between Gunflint Lake, the western starting place, and Whitefish Lake, the eastern terminus, while it is roughly bounded on the north by the Duluth extension of the Canadian Northern Railway, and on the south by the international boundary line west of Port Arthur. Although it is known that the iron-bearing rocks are found over a much larger area than this, it has not yet been determined whether that area

which lies outside the above mentioned limits is not too near the margin of the formation.

It is indeed remarkable that most of the writers on the iron ore deposits of Minnesota have never mentioned, or only in an "en passant" manner, that the same iron bearing rocks extend into Canada. There is a brief notice to that effect in Prof. Van Hise's monograph, "The Iron Ore Deposits of the Lake Superior Region," but we poor mortals on this side of the line can gather very little consolation from his remarks. I hope not to transgress too much upon the patience of the reader if I make a few comments on the notes of Mr. Van Hise. My excuse must be that it has of late been an only too frequent occurrence for me to hear depreciating utterances, the direct sequel of Mr. Van Hise's expressed views concerning that part of our country which is my present subject.

He says:—

(1) "With one possible qualification, as far as one can see, there is no known geological cause why iron ore should not extensively exist on the Canadian side of Lake Superior. This qualification is due to glacial erosion"

(2) "In this paper it has plainly appeared that the iron ore deposits are products of the surficial belt, and that they were formed mainly in pre-glacial time."

(3) "It has also been seen, (pp. 334-336), that glacial erosion, which removed a considerable portion of the belt of weathering, certainly carried away large quantities of this iron ore, which have not been appreciably replaced by the processes of concentration since glacial time."

(4) "The United States side of the boundary is a region of moderately vigorous glacial erosion and very marked glacial deposition. The region on the Canadian side of the boundary, north of Lake Superior, is one of very vigorous glacial erosion and comparatively small glacial deposition." And again: "However, it cannot be doubted that the glacial erosion was more vigorous north of Lake Superior than to the south,"

(5) "In so far as the glacial erosion was more vigorous, just to that extent more iron ore was lost north than south of the lake, and the conditions are to that extent less favorable for the existence of numerous large ore deposits."

(6) "The weight that must be given to this qualification can only be ascertained after extended exploration. Certainly it appears that the rocks in the Animikie area between Gunflint Lake and Port Arthur contain more of the original iron-bearing carbonate and show less of the residual material of the belt of weathering, viz. ferruginous slate, ferruginous chert and ore-bodies, these appear to have been largely swept away, thus exposing the little altered rocks. If in the Mesaba district glacial erosion had cut 150 feet deeper than it did, the larger portion of the ore deposits would have been lost. Moreover, the Gunflint formation of the Animikie district is not nearly so thick as the Biwabik formation of the Mesaba district. It may therefore be doubted whether the Animikie district will ever be so productive in iron ore as the Mesaba district."

This is the position assumed by Prof. Van Hise in the matter. In reply to his arguments let me say :

(1) Mr. Van Hise's hypothesis as to the role played by glacial action is doubtless exaggerated, as far as it is applied to the subject of his monograph, and more particularly as regards the denudation of the iron deposits in our country. This I shall prove more fully below.

(2) That the iron ore deposits are products of the surficial belt will hardly hold good, after a careful field and mine study, as will be seen when my argument is further developed.

(3) Everyone who is acquainted with the formation knows that most of the iron deposits of the Mesaba range are truncated by erosion and glacial action, and besides this we must take into consideration that these iron-bearing rocks have been most probably overlaid by Keewenawan rocks and doubtless also by trap overflows. I therefore fail to see how it is possible to reconcile the statement of Mr. Van Hise that "only a moderately vigorous" glacial erosion has taken place,

with the previous assertion that glacial erosion has carried away a considerable portion of the belt of weathering.

(4) In an examination of the Animikie district north of the United States boundary, we find that large areas of the iron-bearing rocks are still overlaid with diabase sheets, sometimes of considerable thickness, which have protected completely the underlying Animikie rocks, and that, from the time that the trap rock was laid down, no weathering has been possible at the surface of the iron-bearing rocks. Even in such places as have been denuded of this protection, the moving ice has caused but little loss, as in such areas we still see the Animikie rocks almost intact. Consequently we may safely conclude that the effect of the glacial erosion has been far less intense on this side of the line than is claimed by Mr. Van Hise. Why indeed should an imaginary boundary line have been proved such a barrier to the ice sheets, as to lead us to suppose that in Canada they played such havoc, while only a few miles to the south their action has been so considerate and gentle? Nothing lay in their path to divert them to other quarters.

(5) This also shows that there is no foundation to his claim that we have, through this greater glacial erosion, lost more iron than has been lost south of the line. As a matter of fact I can say without any hesitation that not a pound of iron ore from our Animikie rocks has been lost or swept away, and, in direct contradiction to Mr. Hise's supposition, the iron deposits in our rocks should be larger and more numerous.

(6) In our iron ranges, as in all those in the vicinity of Lake Superior, exploration and exploitation are necessary for the verification of the existence and extent of the iron deposits, but surely it is not necessary to devote very minute and exhaustive field-studies to the task of proving that the original iron-bearing rocks, which interest us most at present, are still intact, and that they represent the *upper* and not, as Mr. Van Hise would have us believe, the *lowest* stratum. The "little-altered rock" alluded to by him as being exposed here, is neither the lowest horizon of the iron-bearing rocks, nor is it the same which he believes to be the floor of the iron deposits in the Mesaba. By

this time I presume, Mr. Van Hise has become aware of the fact that his latter theory, concerning the floor of the Mesaba deposits, is also incorrect, and that the true floor lies deeper, and consists of an entirely different rock. This theory has deceived the miners and drillers on that range to such a degree that, whenever they encountered at a certain depth a "horse" or stratum of "jaspilite" they stopped work, while, had they kept on, they would have found the condition for the existence of ore as favorable below these jaspilite sheets as above them.

And, if in some places our district does not show so much residual material, the fact can be explained in the same way, though in most localities this material is not by any means lacking. In fact in one place we had to use over 200 feet of stand-pipe, while in others much less was required. This residual material is derived from the rock which forms the northern margin, where it has been rather shallow but widely distributed throughout the whole Thunder Bay district. These thin rock-layers contained some high grade ore, but in small streaks, changed by the heat of the trap overflow from a limonite or hematite into a magnetite of high Bessemer grade.

I further dispute the statement that the Gunflint formation of the Animikie district was not nearly so thick as the Biwabik formation of the Mesaba district. Had Mr. Van Hise confined his claim to the immediate vicinity of Gunflint Lake, his contention might have been correct, because, at its north and east extremity, the Biwabik formation is almost wholly cut out by younger eruptive rocks, as gabbro, granite and different trap rocks—porphyritic and graphitic diabases—as is also the case at the southern and western part of the same formation. But if he had examined the region eastward from Gunflint, he would have found that the Animikie rocks are perhaps considerably thicker than the Mesaba.

I am therefore compelled to conclude that Mr. Van Hise was not correctly informed, and is therefore not in a position to form an opinion as to whether or not these rocks on our side of the line will ever be so productive as the Mesaba.

As a matter of fact, for which I can vouch from personal observation, we have found iron ore in widely separated localities in the Animikie rocks, to what extent we shall soon learn from practical experience with the diamond drills, which we now have working there. I see no good reason why we should not open up here deposits of hematite ore as large and extensive as those of the Mesaba, Gobeic and Vermillion ranges, considering that the geological and petrographical conditions here are absolutely identical with those of the United States ranges, and that, as I have shown above, glacial erosion has not played the destructive role attributed to it by Mr. Van Hise, whose statement, that we have lost, more or less in their entirety, the hematite deposits formerly existing here, is so evidently erroneous.

I must once more emphasize the fact that these iron-bearing rocks are still intact on this side of the line, and especially those portions which, on account of their greater thickness, hold out promise of containing the largest deposits of ore.

These ferruginous cherts, or iron-bearing rocks, as we might continue to call them, were widely distributed over the archæan rocks, south, north and west of the present Lake Superior basin; not of course, everywhere to the same thickness, but principally in regions where the archæan granites had remelted the older schists. Accordingly we find to-day, these iron-bearing rocks resting upon and filling out every elevation or depression in the former. That these silicious rocks keep so close company with the older acid rocks is significant as to their genetic relation to the latter. In the more elevated portions of the granite, which would be along the present north shore of Lake Superior, and the region north of the Duluth extension of the Canadian Northern Railway, the deposited chert would be thin, and thinned out altogether where the elevation was more than 1,400 to 1,500 feet above sea-level. That these figures are correct, can be proved by field study in many places where the cherts have been protected by trap overflows; in a few other places, where the latter rock is eroded, black slates still cover them, but in general, those black slates are an insecure basis of proof, occurring, as they do, only locally and not

over the whole chert area. Of these thin chert layers a large portion is eroded and carried into the depression southwards. The thickest portion of the chert is south of the above mentioned region, towards the United States boundary, where it rests in a basin to a thickness of several thousand feet. This basin rises rather abruptly near the other side of the boundary line, but through its whole extent the iron-bearing rocks are absolutely intact, and only a very few feet have been carried away in glacial times. It is easy to recognize which is the uppermost part of these rocks, because below the trap the chert has been melted, usually into a red jasper, which shows a "geflossene" structure, sometimes with the most beautiful arabesques or other fantastic designs.

The late Dr. Selwyn, formerly director of the Canadian Geological Survey Department, estimated that in some localities the thickness of the Animikie rocks reach even as much as 12,000 feet. Although I might not go quite so far, yet there is no possible doubt that in some places this rock formation is considerably thicker than the Mesaba.

As above stated, the present depressions in our Animikie rocks are everywhere overlaid with a residual material, exactly in the same manner as in the Mesaba. This material, I may repeat, came from the northern margin of the iron-bearing rocks, and is usually deepest where the ice-sheets butted against a high, resisting hill, while upon the south side of such hills the deposits are shallower and the slope is not so precipitous as on the north.

Numerous dykes have cut the iron-bearing diabase, also gabbro and granite; but the two latter can be observed occurring more frequently in the vicinity of and east of Gunflint Lake. These intrusions served beneficially in two ways, first, in loosening the chert strata, and second, in forming troughs of impermeable rocks, which permitted the meteoric waters to enter more freely and remain longer in contact with the iron-bearing rocks, thereby allowing the chemical changes to be performed more rapidly and thoroughly.

Now, what are the constituents of these iron-bearing rocks? Siderite and a cherty, silicious substance, in layers, varying in proportion from one per cent. to almost pure siderite. These materials are always

mechanically mixed, not chemically combined. Where the latter should happen to be the case, it is the result of metamorphism or contact metamorphism.

At this point I will repeat in a synoptical form the conclusions arrived at and explained at greater length above :

(1) That we have here in Canada the same iron-bearing rocks as exist in the Minnesota and other Lake Superior iron ranges.

(2) That this rock formation is as thick, if not thicker than in the other localities.

(3) That the conditions for the formation of iron ore deposits here are identical with those elsewhere.

(4) That there exists therefore no good and sufficient reason why there should not exist as large, if not larger, iron deposits here as in the Mesaba or other districts.

GENESIS OF THE ANIMIKIE IRON-BEARING ROCKS.

Allusion was made above to the occurrence of an iron range in the Thunder Bay District, on the north-west shore of Lake Superior, similar to the Mesaba, and I expressed my confidence in the possibility of finding here as large, if not larger iron deposits than the latter. I will now devote some consideration to the genetic relation between these iron-bearing rocks and the rocks upon which they rest, and also to the relation in which the iron deposits stand to the rocks in which they occur.

In order to facilitate the correct understanding of the position I hold with regard to this subject, I shall divide this paper into the following heads :

- I. Geology of the region.
- II. Area upon which the iron-bearing rocks were laid down.
- III. The constituents of these rocks.
- IV. Their genetic relation to other rocks.
- V. Their metamorphism and causes.

I.—Geology of the region.

Under this head I will describe the stratigraphical geology of that district where the iron-bearing, that is the Animikie rocks are

found, and how they are produced, and to which era the different series belong.

I might mention, by way of introduction, that it will not prove a task of much difficulty to describe these various rocks as they are found here, that is laid down in their true sequence, but the most difficulty will be found in convincing the reader as to how they were produced, and under which geological period they must be classified, since, in regard to these points there exists such a divergence of opinion.

I shall therefore abstain from quoting the numerous written opinions on this subject, but shall consider principally those whose authors are intimately acquainted with the geology of our country, and whose writings have been founded upon personal observation and study. Not that I wish to belittle the opinions of other writers, but I believe that preference should be given to the version of those of us who have lived for years, and are still living among these rocks, and have devoted considerable time to their study, and endeavored to solve the mystery of their genesis. We also, being on the ground here, are naturally in more favorable circumstances than those who reside in localities where this rock formation is less fully exposed for study and whose theories on this subject must therefore always be to some extent speculative. Our earliest writers on this subject labored under the disadvantage of having to cope with a country, new, uncultivated, undeveloped and overgrown with a dense vegetation, through which the trails followed by the white or Indian trapper, and the waterways travelled by their canoes, were the only paths then open. These early pioneers of science could only obtain very fragmentary knowledge of the country, and lacked the means to weld it into an harmonious whole.

Even at the time of my own arrival, 15 years ago, there were many localities where no white man had ever set foot. All this is changed. Large clearings have been made for mining and agriculture, and still greater areas have been cleared by fires, which for years have swept the district, destroying not only the vegetation, but even the humus which covered the rocks, leaving them in many places absolute-

ly barren. What does all this reveal to us? The plainest picture of the early history of our globe to Cambrian (?) times, and what we learn to read from this is expressed in a few words: "That the Laurentian gneisses and granites are not the oldest rocks of the crust of our globe, but that they form the base for the most ancient rocks—our schists—and that both are overlaid by pre-Cambrian sediments and divers eruptives."

I give them in tabulated form below, but I might first mention that there are some divergences between the Canadian classification and that of the United States Geological Survey. I therefore tabulate them both here, and shall speak later on their differences.

CANADA.

Archean.

ARCHEAN.	Huronian.	(Upper division)	{ Chlorite schists. Hornblende " Felsite " Sericite " Altered trap and quartz porphyries.
		KEEWATIN	
		(Basic)	
		(Lower division)	{ Mica schist and finely laminated gneiss.
	COUTCHICHING		
	(Acid)		
	Laurentian.	(More basic)	{ Hornblende granite. Gneiss and hornblende syenite gneiss. Biotite, g. anite-gneiss (granitoid) and granite.
		LAURENTIAN	
		(More acid)	
Cambrian.	(Animikie)	{ Flat bedded cherts, carbonaceous argillites (?)	
	(lower)		
	CAMBRIAN		
	Nepigon	{ Dolomite, sandstone, with intrusions of gabbro, granite, trap in dykes and sheets.	
(upper)			

UNITED STATES.

Archean.

Granite and Gneiss.

Pre-Cambrian, Algonkian.

Huronian (principally sedimentary) * including the iron bearing rocks like our Animikie, further, quartzites and the folded schists of Canada (!)

KEEWENAWAN.

Trap sheets and sandstone.

(*NOTE.—As the list of Huronian rock names is too long and the same rock is often given half a dozen different names, also a number of them are claimed to belong to the Archean, it is most difficult to give these rocks in their true order.)

We see by this table that the Canadian Geological Survey has two divisions in the Archean series, an upper and lower, correctly so, while the United States Survey has only one, as far as it is possible to find anything amid this chaos of classification and names, viz.: the Laurentian granites and gneisses. Further the Canadians place the pre-Cambrian of the United States among the Cambrian; whether this is correct, is questionable. However, we might argue that when the United States place the oldest rocks of our globe among the sedimentary rocks, and appear to consider that they belong there, the Canadians have just as much right to classify the pre-Cambrian among the Cambrian, having the further justification that they believed that they had discovered fossils in these rocks. Let us now examine these classifications and commence our criticism with the United States method of classification of the Archean.

During the course of investigation in our field, and comparison with other localities on this and other continents, I am convinced that in no other country does there exist a more simple or plainer exposure of the oldest rocks that compose our earth's crust. This applies especially to the Laurentian series, which represent the greater portion of the rocks of the country. There is nothing to mar a clear view, or to prevent easy study, for no younger sedimentary rocks are overlying them, at least not in sufficient volume to prove a disturbing factor. Next in importance are the oldest of the rocks known to geologists,

the metamorphic schists, which are steeply tilted and quite crystalline. Were we to plat these rocks, they would form on our diagram large and long-drawn-out islands in an immense sea of Laurentian areas. Now as to the relation that these bear to each other. In brief, I may say that the Laurentian rocks form the base of the schists; whenever the latter are eroded we can see their imprint on the former. We further observe that they rest unconformably and sharply defined upon the gneisses and granites, but we also notice the latter rocks as dykes, intruding in great numbers into the the schist strata. These schists represent the remnants of the former crust of the earth, which was almost entirely remelted when the viscous Laurentian magma again became eruptive and fluid, broke up and absorbed this thick crust. Later on a more gentle flowing motion must have prevailed, through the influence of which this magma "underflowed" the older rocks that were still remaining, melted grooves and channels on their lower side or base, leaving the projections protruding downwards like knife-blades or stalactites, into the magma, as we find them now bedded into the solidified gneisses. In the course of ages, portions of the schist have been eroded, the glaciers sweeping away the residua and polishing the surface of the gneisses, so that we see remnants of the schist still resting in alignment with the foliation of the gneiss. In certain places, where these softer remnants were more completely eroded out of their harder resting place, the surface of the gneiss has the appearance of filigree work. That the schist-remnants, resting in the gneiss, usually appear in line with the foliation of the gneiss, is perhaps a certain indication of the occurrence of the flowing motion, mentioned above, of the latter in a certain direction. A further confirmation of this is given by the arrangement of the mica, mostly biotite, which, it seems probable, in the last stage of the flowing movement of the gneissic magma, had already crystallized out of the mass, and the different particles arranged themselves so as to follow each other along the line of the least resistance. This arrangement in many cases gives these rocks a foliated appearance which is however only an optical illusion. The gneisses are only really foliated, or appears chistose, or show tectonic metamorphism, in places where they were disturbed by the younger eruptive rocks or came in contact with them.

Without going into a detailed description of the petrography of these old Laurentian rocks, for which I refer the reader to the reports of the Canadian Geological Survey, especially to Dr. A. C. Lawson's report for 1887, Part F, I might merely mention that they consist in part of basic rocks,—of hornblende granite and hornblende syenite-gneisses,—and in part of acidic rocks,—of biotite gneiss and biotite granite-gneiss. One peculiarity of the distribution of these gneisses is that usually the more basic is found in the neighborhood of the more basic series of the old Huronian, the Keewatin hornblende schists, while the more acid, very frequently, but not invariably, is found in the neighborhood of the more acid series of the Huronian, the Coutchiching mica schists. I mentioned above that, through the arrangement of the particles of the dark biotite it is often difficult to distinguish between them and the really foliated gneiss and it would be a hard matter to prove that they were not one and the same rock and not of idiogenetic occurrence. The same may be said of all the Laurentian gneisses, for they merge imperceptibly into one another. In many places we observe intrusions, larger or smaller, of a younger granite, either as dykes, apophyses or large circular or elliptical areas, which are often mistakenly classed as belonging to this same early period. These granites, although sometimes also gneissoid, followed a period of basic eruptives, such as gabbros, diorites, and peridotites, with which in some localities they are found in close connection, even intruding into them, and they are therefore younger. A further proof that these granites are a more recent formation, is the fact that they often contain large angular inclusions of gneiss, sometimes ten to fifteen feet in length, on the top of hills hundreds of feet above the level of the gneiss.

Very frequently we find fragments of the old Huronian rocks, either angular or rounded by melting, resting in the gneisses, usually at no great distance from the contact with the former, though these are also occasionally observed removed some distance from the point where they dropped into the gneissic magma. For further information on this occurrence, I again refer to Dr. Lawson's report cited above.

The brief description of the old Laurentian rocks given here, together with reference to the sources given for more detailed informa-

tion, will or should convince even the strongest adherent of the sedimentary origin of these gneisses that there is no truer igneous rock in existence than our Laurentian gneiss, and they are also found in other regions occurring exactly as they do here, whether they are obscured or not, covered by younger sedimentary deposits, or metamorphosed through one cause or another. In the following pages we will have still further proof of their origin, and see what light they throw upon the oldest rocks of the earth's crust.

Geikie, in his *Text-book of Geology*, page 1156, asks, "whether these gneisses were ever connected with any superficial volcanic activity, although no such connection had yet been ascertained, but it might be regarded as highly probable." In my opinion, volcanic activity, in the sense of a lava or magma extruding from the vent of a volcano, was not necessary, or possible, for the largest portion of the crust was remelted, and there was nothing therefore to confine the gases, fumes and lava, and to compel them to seek, perforce, an artificial outlet. The later granites, on the other hand, show at many places signs of volcanic activity. Some of these volcanoes show different emanations at different periods, as in one a granite, in another a gabbro and again in another an acid rock, and so on. These successions of basic and acidic rocks seem to have been the rule during the earliest history of our globe, this phenomenon being plainly seen all over the country. The sequence has been :

- (1) First crust, (I should have said the first of which we have any knowledge), Basic ; our Huronian rocks.
- (2) Second crust and intrusions into No. 1, Acid, Laurentian rocks.
- (3) Third eruptions and intrusions, Basic ; Gabbro, Diorites, Peridotites and Traps, not specially classed, usually wrongly grouped with No. 1.
- (4) Fourth eruptions and intrusions, Acid ; Granite often wrongly classed with No. 2.
- (5) Fifth eruptions and intrusions, Basic ; Diabases, porphyritic, graphitic, amygdaloidal and typical.

When the Laurentian gneisses and granites began to solidify, they

contracted, and in consequence the overlying, older rocks, being forced to follow their movements, were steeply tilted and compressed, this giving them their slated, foliated and crushed appearance, and naturally furthering their metamorphism. A number of writers claim to recognize in this slating and foliation, a proof of the sedimentary origin of these rocks, but I am convinced that their view is erroneous, for when we consider the facts as set forth above, deduced from the clearest field evidence, the only possible conclusion is, that these rocks are the result of the cooled magma which surrounded our globe in the earliest stages of solidification. It were unwise to affirm that this was the first crust of all, for there may have existed many crusts long previous to this which have suffered the same fate as did the one which we are considering, through the eruption of the granites and gneisses, but so much more thoroughly as not to leave a trace behind. Where, indeed, could these sediments have come from? When we realize that these rocks themselves were resting upon an exceedingly hot, viscous magma, would it have been possible to float a sediment with water on top of this fiery, molten mass? Where could the water have come from in sufficient quantity to carry out this process? At that time water could only have existed in the immediate proximity of our globe in the form of vapor, and, even if such vapors should have become condensed on the outer sphere, the resultant water could not have reached the surface as such, for the heat radiation would be too great for such condensation, and would have made absolutely impossible any accumulation of masses of water upon the surface. There is no basis for the hypothesis that the Huronian rocks are the product of sedimentation, their foliation and schistosity forming no argument in favor of such a theory, these being matters of every-day observation in even the most solid eruptive rocks, wherever they have been subjected to the same stresses as have produced this appearance in these Huronian rocks.

This being the case, the practice adopted by the United States Geological Survey of classifying these Huronian rocks of ours among the sedimentary and Pre-Cambrian, is without doubt peculiarly erroneous, and I do not see any justification for casting ridicule upon our own classification, which groups them among the Archaean, and calls

them the oldest ones of that series. Everyone knows that the name, Huronian, was applied by Logan to the rocks occurring on the north shore of Lake Huron; granting that a large portion of these are sedimentary, we are also well aware that the older metamorphic schists are not lacking there, and if we further consider that these latter rocks are the dominant representatives of their class in western Ontario, while the younger sedimentary rocks show only in isolated areas, why are we not justified in giving the former this specific name? Our practice has far greater right and correctness than that of the United States, which names as Huronian what is not Huronian, as for instance the Animikie series, which does not exist round Lake Huron; still in all monographs on the Lake Superior iron region we see them classed among the Huronian, and the same rock is given possibly a dozen different names. Also among their Huronian rocks appear the Canadian folded schists, which means that they are—horribile dictu—considered sedimentary!

If the Canadian Geological Survey classifies the Animikie as lower Cambrian, this should fit in with the hypothesis of the American geologists, who claim that a number of the minerals in these rocks, carbonate of iron, lime and magnesia, owed their origin to the presence of life in this primordial ocean. (I shall discuss this further below.) Should this be correct, I see no good reason for us coining a new period, "Pre-Cambrian," out of these rocks, when we know that the Cambrian age is the one in which we thought to have detected the first existence of organisms. There is doubtless a certain incongruity in the determination and classification adopted by the United States Geological Survey with regard to these, the oldest rocks of our crust. If we wish to adopt a new name for these rocks, then we have to do away with the organic life theory, and the quicker we do this the better, because it can not be supported by facts. The reason for this will follow hereafter.

Now if the placing of the Animikie rocks in a new geological era was incorrect on account of connecting them with organic life, then the Canadian classification is also incorrect, because we have no proof of animated organism existing at that time. Logan once found in a

dolomite a peculiar design whose structure was somewhat similar to that of a foraminifer, at least Sir W. Dawson thought that he recognized it as such. This view was later disproved by a number of observers, but I think that the most effective argument in opposition to it is furnished by these rocks themselves, which have never yielded anything in corroboration of it, although they have been prospected and mined for years and eagerly and carefully scanned for traces of Logan and Dawson's "Eozoon." Can we conceive of the possibility of the existence of organic life, when we consider the origin of these rocks, which were laid down in a time of almost uninterrupted volcanic activity, during which there appeared, in the following order:—

First, the cherts and carbonate of iron, deposited by hot, mineral springs, and geysers.

Second followed the eruption and intrusion of gabbros, diorites, peridotites, traps, etc.

Third, took place the eruption and intrusion of granite.

Fourth, followed intermittingly, diabase eruptions and intrusions, and, in their last stage of eruption, overflows over the whole Cambrian area.

Fifth, might be mentioned the deposition of tuffs and ashes, from (4), which seem to have always preceded the extrusion of the trap magma.

When we see the great number of dykes and the crater-like structure of hills, both large and small, and the heavy overflow of trap magma, the deposition of which was distributed over a considerable period, we cannot help coming to the conclusion that it was utterly impossible for organic beings to have existed at that time. Therefore we have to exclude this hypothesis from our theory of the origin of the minerals occurring in these rock-formations, and also to dismiss the idea that the Logan "Eozoon" was an organism. This may be yet better recognized after following my views regarding the origin of the chert, slate, etc.

Following the sequence of these rocks as they were laid down, in an ascending order, we come now to the next rock above the granites and gneisses, which is a ferruginous chert, found compact, or as

granules, varying in size from that of granulated sugar to that of a pin's head or even of a pea. If in the latter size, we often find the largest individual granules filled either with iron carbonate, or lined with whitish quartz in vug-like fashion. In color the chert is from white and yellowish grey to dark green and black. Always associated with it is siderite, sometimes intimately mixed, but more often surrounding the chert granules as a cement, also in separate independent layers, and again mixed with the cherty silica in almost every percentage. This variety of form, of mixture and of color, is so great that it alone would provide subject matter for a voluminous paper. Not infrequently we also meet with a red jasper associated with the chert, but this is, in most instances, if not invariably, produced through the heat of intrusive rocks, for we find this jasper principally in the neighborhood of these, and especially below the trap overflows, where it often shows a flowing structure in most beautiful designs.

As secondary minerals there appears here and there in small fissures, calcite, and in the vicinity of basic rock intrusions, some iron pyrites.

The thickness of the ferruginous chert rock may be estimated as being, in some places, 2,000 and possibly 3,000 feet, but not, as was taught some years ago, 12,000 feet. In most localities the thickness is not more than 500 to 1,000 feet.

As oxidation products of the ferruginous chert we find magnetite, in smaller or larger layers, but principally hematite, either in large, flat-lying beds below the drift, or in troughs of the rocks which form the base of the chert. The magnetite was formed from hematite, or limonite, by the heat of the intruded or overflowed rocks, by exclusion of air, and it occurs often very pure and of extremely high grade, which is characteristic of ore formed before the eruption of the various later rocks. Later I shall have more to say about these rocks and minerals.

Next in order would come the locally so-called black slates, also and perhaps misnamed Argillites resting conformably on the former. These rocks are, doubtless, the solidified beds of ashes and tuffs produced by explosions and exhalations, which formed the prelude to the trap eruptions in Pre-Cambrian, and possibly also in Post-Cambrian

times. These slates are not found everywhere, and are in many places only very thin, in other places, and these are principally near the larger vents, they are thicker, but seldom more than 300 to 400 feet. Their aspect in some localities is quite black, in others from light to dark grey in color, if so they have the same shade of color as the diabase covering them; they are in some places foliated, but are too soft, that is, not strong and elastic enough for roofing slates but have the tendency to break in cubes; in other places they are compact and have only a faint foliated aspect. The black color in the darkest variety is produced partly by iron and partly by graphite. Some writers present this graphite as evidence of the existence of organic life in that "primordial sea." I think that I have already given sufficient proof that this can not be the case. We have to look for another source, and this source is found in a number of trap dykes, which are intimately and heavily mixed with graphite. In examining these closely we find that they are plutonic rocks of coarse crystalline structure, and exposed to the surface through the erosion of the chert and later trap overflows, so that there can be no question of an infiltration of carbonaceous matter in solution. Upon observing this graphite in the trap, by a natural inference one would be led to consider whether it would not be just as possible for the graphite in these slates to be an emanation product of the trap volcanoes as the black slates themselves. If it was possible for the graphite to be sent forth in a magma stream, then it should be no impossibility for it also to have been mixed and thrown out with the ashes and tuffs coming from the same source. With our present knowledge of volcanoes and volcanic phenomena, it surely should no longer be necessary to connect every little particle of carbonaceous (in this case, graphitic) matter with organic life; how many are the tests that have been made from the gas exhalations of volcanoes; how often is graphite found in eruptive rocks, under conditions that preclude the possibility of organic origin; why then jump to conclusions which are applicable under no circumstances, and least of all in this case? Surely it would be outside the bounds of reasonable speculation to affirm the theory that all the carbon gases which our globe contained during the earliest periods of its history were ejected into space, and that from this

space the later-appearing organic life had received its carbon compounds, and that again these organisms had furnished the carbon for inorganic substances, such as minerals and rocks. From whence then these graphite contents in plutonic rocks, these diamonds in igneous basic rocks, whence the carbonic acid and oxide gases in the exhalations of volcanoes and in the pores of igneous acid rocks? Moreover, if we believe that other worlds are composed of the same material as ours, from what source did the carbon in meteorites come, as an alloy of iron and nickel? Surely nobody will claim that these are composed of a mixture of metals and organic remnants. All the examples cited above should lead us to regard the origin of the graphite in these slates to be different to what has been supposed by some writers, nor should we apply that easy, less mentally exciting method, the organic theory, more especially when we are dealing with a geological period which was a time of almost uninterrupted volcanic activity. Yet further, if we are doing away with the organic-life theory altogether, we shall have to consider the origin of the calcite also, in these black slates, from a somewhat different point of view to that taken by these other writers.

Silver mining in the black slates has furnished us with the clearest evidence as to the origin of the calcite. Wherever we mined in the neighborhood of, or alongside a trap dyke, the veins were not only stronger but also better—in fact, I might say in most instances, only at such places—mineralized with argentite and native silver, sphalerite, chalcopryite and galenite, and with rock minerals, principally calcite, also fluorite and barite, with only here and there some quartz, which appeared, in most cases as a secondary product. At greater distances from the dykes the veins carried only the rock minerals. Fumaroles alongside the trap were evidently the direct cause of the filling of these veins, for even at the present time warm mineral springs were tapped in the lower mine workings, along the dykes. Of these the Silver Islet spring was especially noteworthy, also those in Crooks township and Medicine Bluff, all of which are quite saline in character. The Silver Islet spring was tapped at a depth far below the level of the slates and cherts, therefore the lime contents of these waters could not have their origin in the latter rocks, as might be inferred from Bulletin VI.

Minnesota Geological Survey, p. 145. That these lime waters intruded also into other rocks, that is in the planes of the slates and small fissures of the cherts, is easily understood, and may likewise be recognized by examining the drill-cores coming from every depth of the formation. Originally, these slates may have been free from lime, and so most assuredly must have been the cherts, as can be sufficiently proved at places where the lime water had no admittance into them. If the lime had been produced in a manner similar to what some have taught us, we would see the calcite not only filling the interstices of the rock, but we would find an intimate mixture of lime with the other rock constituents. More than this, we would not see an exclusively coarse, crystalline vein product, but rather a fine-grained or crypto-crystalline, almost amorphous looking lime rock, such as we see in the younger sedimentary and fossil limestones. And we would, further, not only find large, true fissure veins, of considerable width and strike, compactly filled, but we would also notice in the slates and cherts lime rock layers, especially at places where there were no rock fissures to be filled. Instead of this we see, even in the highest elevations of this region, immense calcite veins in localities where there are no slates and very little chert, but only granite and trap, and where a draining process into fissures, from the directions in which slates are resting, was an impossibility. And why should the metallic minerals have kept so closely in company with these trap dykes, if they were not brought up by the fumes and vapors escaping from a cooling magma, or even from a deeper source? These, reaching the upper strata of the earth's crust, mingled with the waters flowing into those wide fault-fissures of the Animikie rocks, and, being forced upwards by the difference in the isogeotherms, the minerals were deposited in the cooler rock strata, in the slates first, and later in the cherts and granites, according to the shifting of the isogeotherms downwards. I might yet mention that we find these calcite veins almost exclusively below the last trap overflows, and only in a very few places do we find small stringers of calcite, fluorite and amethystine quartz filling little fissures at the surface of trap deposits, which gives us proof that the thermal waters above described had force enough to reach even the highest points of these hills.

After the period of the deposition of the Animikie rocks the lake in which the former were laid down must have subsided before the filling of these calcite veins, for it would otherwise have been impossible for these veins to have been formed in the manner in which they were evidently produced. The subsidence of these lake waters from over the largest portion of the former lake area is easily understood, when we see the filling up of the lake bottom by sedimentary as well as eruptive rocks, and take into consideration the sinking of the present Lake Superior basin, which occurred very likely after the principal trap eruptions.

Whether the upper part of our so-called Cambrian rocks, the Nepigon or Keewenawan series, were laid down in that same lake before the subsidence of its waters, or whether they date from a later period, after the waters had refilled that part in which they were laid down, is a matter rather difficult to substantiate, for these rocks do not occur everywhere over the Pre-Cambrian rock area, but only in the extreme eastern portion of Thunder Bay district; it seems to me however that the latter hypothesis is the more probable, for these Nepigon rocks rest unconformably upon the Animikie, and the material of which they have been composed must have been derived from a totally different source to the material that forms their close surroundings; it came possibly from the north-east and from the same sources that have contributed to the rocks upon the north shore of Lake Huron or in Eastern Algoma, with which the Nepigon rocks are doubtless identical. One exception is found here in the sheets of amygdaloidal trap.

Before I commence enumerating the different rocks of the Nepigon series, I have to speak shortly of the numerous intrusions of eruptive rocks in the strata of the Animikie series. The first to make its appearance was a gabbro, the next a granite, which is usually found in close company with the former, and the third, a diabase, which was especially voluminous at different periods and, very likely on account of its very fluid condition, not only formed dykes, but also intruded as sheets and laccolites between the slate strata, and finally flowed in considerable masses over the top of the whole Pre-Cambrian rock formation, solidifying there in columnar form. On the north shore of

Lake Superior, especially at Nepigon Bay, several sheets of amygdaloidal trap are found below and between the sand stones which are the northern margin of a great number of such sheets passing through Lake Superior, coming up on Isle Royale, dipping into the water again and re-appearing to the south on Keewenaw Point. These trap sheets contain some native copper.

I come now to the upper Pre-Cambrian rock series, consisting of:

First, a jasper and quartz conglomerate, resting unconformably on the slates, being 4 to 5 feet in thickness.

Second, a sandstone followed by a dolomite, or more often in alternating layers with the sandstone, which is sometimes of grey or reddish color; the same variety of color prevails in the marl. Here and there we also find in these rocks intrusions of trap, but the principal mass of the latter rock rests, as I have repeatedly mentioned above, on the top of the whole series of Pre-Cambrian rocks, and is therefore the youngest member of this, or of any other formation in this country. I must not forget to make mention of another conglomerate, a boulder and pebble conglomerate in which the cement was a trap lava, seemingly of the same age as the last trap eruption, and occurring only at a few places near the north shore of Lake Superior. The thickness of the various Nepigon rocks is as follows:

Jasper-conglomerate, 4 to 5 feet.

Sandstone and marl, 200 to 300 feet.

Trap overflow, over the whole, 200 to 300 feet.

The upper series of the Pre-Cambrian rocks is, like the lower, absolutely free from fossils; in vain have we searched for even the smallest trace, but have not so far been fortunate enough to verify Logan's find, although we have actually been working under more favorable conditions than he, being able to see the rocks, not only more exposed, but also more opened up by mining operations. Over and over again prospectors have brought in pieces of float limestone containing fossils, but in every instance these proved to be strangers to this district, and came with the ice from northern regions.

I considered it necessary to make this long digression into the geology and petrography of this district in order to make the subjects

that I next deal with better understood, that is to give a key better fitted for the solution of the problem of the origin of the iron-bearing rocks and also of the iron deposits themselves that occur therein. A still further reason was the divergence of opinion that exists between the Canadian and the United States Geological Surveys in respect to the classification of these oldest Archaean and Pre-Cambrian rocks, so that I felt the necessity of treating this subject at somewhat greater length than was my first intention. If, however, in the foregoing pages I have offered anything which might tend to bring both sides and their views into harmony, I shall rejoice to think that my efforts, which were perforce curtailed by lack of space in the limits of a short essay, have not been in vain.

II.—Area upon which the Iron-bearing rocks were laid down.

The area upon which the iron bearing rocks were laid down must have been considerably larger than that which Lake Superior now occupies. This area was most extensive towards the south, west and also north-east, but probably did not extend far to the east, where the margin seems to be reached not farther than longitude $86^{\circ} 30'$, for we find here numerous remnants which were protected by our Nepigon rock series (Keewenawan), and which show the cherts in that region to be only a few feet in thickness, resting upon Archaean schists in close proximity to the Archaean granite. Near Thunder Bay they do not extend farther than 10 miles north of Port Arthur; this margin can be followed in a nearly straight south-westerly course, skirts the Giant's range to the south, and reaches its extreme westerly limit about $93^{\circ} 30'$ longitude, while its southern limit is not far over 46° latitude. This area formed a basin, but where its deepest part was situated is obscured through the further sinking of the present Lake Superior floor, which must have occurred at the time of the trap eruption. The extreme margin of the iron-bearing rocks was always at an elevation of 1,400 to 1,500 feet above the sea level, as we can see at numerous places where they were protected by trap overflows or sedimentary rocks of the Nepigon series.

It is hardly necessary to affirm that this basin was filled with water, and was therefore a lake of considerable size and depth. If we consider

that this water was resting in a syncline in the Laurentian granite and gneiss, and it was therefore on a heat-radiating rock surface, it must doubtless have been warm, or even hot.

III.—*The constituents of these rocks.*

It will be easily understood that for long periods after the solidification of the granites, a fumarotic and geyser-like activity must have existed, and that the contents of these volcanic springs were laid down in the bottom of that lake as an amorphous silica, and the iron precipitated as a carbonate. They were formed into concretionary sediments, and exhibit all the forms of geyserite and hyalite, or present a pisolitic structure, the latter form predominating, as can be plainly noticed if the ground mass, or cementing mass,—usually siderite—becomes soft or oxidizes; after breaking the rock we have an oolitic picture, the little granules remaining whole, so that some of them, consisting principally of ferric oxide and silica, can be lifted out of their resting places with a pair of fine tweezers. Both the siderite and silica appear very unevenly distributed throughout the whole formation, sometimes the one and sometimes the other predominating. This might have been caused, first, partly by an uneven production; secondly, partly by the pressure of the geysers, whereby a violent movement of the waters was produced; thirdly, by a wave-like movement of the water, compelling the lighter particles to follow these different motions. Hardly any other way is conceivable by which these ferruginous cherts could have been laid down, as I have mentioned above, than by "volcanic springs," perhaps principally in geyser-like form. If they were sediments, carried there by water-courses, we should find different rock minerals mixed with them, but this is not the case. This occurred only in the last stages of the production of the chert deposits, when the material for the black slates was laid upon the top of them. Geysers of hot silicious solutions, containing more or less iron, very readily suggest themselves, when we see the many remnants of old geyser vents in this country, of which one stands out as especially remarkable and in an excellent state of preservation. To-day we still see its walls as perfect and smooth as if its activity had ceased only a short period ago. And geysers and the action of geysers should surely be nothing

remarkable in the early history of our globe ; how many localities show signs of them and how many are still in activity ? Let us look at the action of the geysers which silicified those Arizona forest giants into the most beautifully agatized and jasperized wood, or of those at Yellowstone Park. Although the latter cannot be compared in age with the former, I do not see any good reason why, in the earliest history of our globe, similar phenomena should not have occurred, especially so shortly after those immense changes in our earth's crust had taken place, that is, at a time when the Laurentian granites became eruptive, the larger portion of the former crust was remelted, solidified and contracted and sent forth from its magma hot gases and fumes for long periods thereafter. These came into contact with the surface waters, percolating or even flowing through the larger and smaller fissures of these rocks, which took up the contents of these gases, or even the differentiated silica, or dissolved the silicious contents of the granite. Naturally in this case we have to reject the idea of capillary percolation, because the heat in the rock would have been too great to permit a capillary descension of water. At shallow depths these waters would have been rejected in the form of steam, and fissures, produced by contraction, faulting or shearing, would give the meteoric waters an opportunity to flow in, and they would be again ejected in a similar pulsating manner, according to the shifting of the isogeotherms downwards, as is the case with the Yellowstone or Icelandic geysers to-day. As a further proof that the water in this lake was hot may be cited the total absence of even the lowest form of animal or plant life. The fallacy of Sir William Logan's discovery of the "Eozoon" in the black slates or Dolomite has been shown in the earlier portion of this paper.

I think that it would be merely carrying "owls to Athens" for me to adduce any more proof that the present iron-bearing rocks were laid down by numerous hot mineral springs in a lake or sea of considerable extent ; and their contents were derived from gases emanating through fissures and vents out of a cooling granite, and also partly from differentiating silica, as well as from parts of the same rock from which these waters dissolved the silica.

Considerable time must have elapsed from the beginning to the

ending of the production of the ferruginous chert, after which followed the sedimentary deposition of the black slates, from ashes and tuffs of the trap volcanoes. These slates were seldom thicker than from 300 to 400 feet, and were succeeded by a quartz and jasper conglomerate of a thickness rarely over four feet. The next deposition was a dolomite or marl, interstratified with larger and smaller layers of sandstone, both together seldom over 200 feet thick. All these sediments must have been very unequally distributed over the chert area; in some parts they were very likely absent altogether. Judging by the occurrence of trap in other localities, this, the youngest rock in our country, must have been produced in post-Devonian times, after the deposition of all the sedimentary deposits, which it overflowed in varying thickness.

IV.—Their genetic relation to other rocks.

How much this thickness has been it is hard to say, for no other younger rock exists to tell us its whole history. We see only remnants left, of which the most voluminous is about 200 feet thick. Although we see the dykes of this rock in large numbers almost everywhere, it is very questionable, however, whether the emanating lava found a region level enough whereon to rest in an even thickness, also whether this lava was produced in the same volume by every dyke; this would, however, be impossible, for there are large and small dykes which must have varied greatly in their capacity for production. Exactly as we notice hills and dales today, so at that period there must have been elevations and depressions. It is hardly conceivable that erosion alone should have produced those existing at present, they were merely intensified by the scrubbing and grinding of the icebergs which freed the rocks from the eroded products. In our country this erosion was not vigorous enough to reach the iron-bearing rocks to such an extent as to give us cause today to mourn the loss either of our iron deposits nor yet of the material which would make further deposition possible, as has been sometimes argued.

V.—Their metamorphism and causes.

Now it will be seen from what I have stated here, that there were places, after the eruption of the trap, where the overlying material

could have been no barrier to prevent these meteoric and mineral waters from percolating into these iron-bearing rocks, and producing such changes as would be necessary for the conversion of the ferruginous chert into iron ore.

Before I commence on my version of the genesis of these iron deposits, let me first recapitulate here what Mr. Van Hise says regarding the process that took place, which he sums up as follows :

(1) The principal part of the iron is derived from the belt of weathering, consisting of detrital ferruginous sandstone and conglomerate.

(2) To some extent also, it is derived from the oxidation of the iron carbonate in place.

(3) All the facts, however, lead irresistibly to the conclusion that the final and most important step in the production of ore bodies was secondary enrichment by downward percolating waters, below crests or slopes, where such waters were conveyed by the pitching troughs.

(4) Waters which followed the more circuitous route transported iron carbonate ; waters more directly from the surface which did not pass through iron carbonate, bore oxygen ; the two kinds of solution mingled and precipitated iron oxide. The waters ascended and escaped below the valleys, and finally the great quantity of water which was converged in these troughs and moved downward, abstracted the silica and carried it elsewhere.

Theories founded upon facts are incontestable. but the practice of modelling facts—*nolens-volens*—into the frame of a preconceived theory, antiquated or not, is dangerous, and only too often serves as a shelter to hide one's true conviction, for fear of criticism, just or unjust. Either we have to shelve a theory fearlessly if it is useless, or we have to remodel it so as to fit the facts. What about the above ? Consideration of clause (4) alone would tear the fabric of this peculiar theory all to shreds. Imagine great quantities of water moving downwards, of course first through the belt of weathering, which is said to have furnished the principal source of the iron ore deposits, but not extracting any of the iron said to exist there, and leaving all the carbonate of iron in it, extracting only the silica and disappearing, trace-

less, with this. Next comes a second inflow of water which extracts all the iron from this belt of weathering, and carries it into the spaces prepared for it by water No. 1; at the same time there comes, from another side, water No. 3 which, going through quite different channels and fissures, not detected by either water No. 1 nor 2, collects carbonic acid and carbonate of iron, then meeting water No. 2 exactly in those prepared spaces, frees it from the iron, which settles down in that water-filled trough, and waters No. 1 and No. 2 disappear behind the scene. Another peculiarity is that this same process must have been going on in all the ferruginous chert deposits around Lake Superior, in exactly the same manner, viz. in Mesaba, Vermillion, Gogebic, Menominee, Marquette and here, because the iron in all of them is derived from the same rock material.

Let us now dissect this whole process a little more in detail, and if the reader will take in hand Prof. Van Hise's Monograph on the iron ore deposits of Lake Superior, and will look at the sections of the mines and follow my comments, he will soon be able to judge whether Mr. Van Hise's theory is correct or not.

In the first place let me say that this ferruginous chert or iron-bearing rock is a very compact, and, owing to its constituents and structure, usually a very hard rock, and it was very little affected even by the many eruptive rocks, such as gabbro, granite and trap, for we see it seldom dipping more than 12 degrees, and in most cases but 5 degrees from the horizontal. Even in the neighborhood of these eruptives it was but little disturbed, and seldom showed large fissures through which great quantities of water could have rushed in. Perhaps we must except some fissures which were produced here and there at the time of the trap eruption, through faulting; these however were soon closed again by the fumaroles of this rock carrying in calcite, baryte, fluorite and amethystine quartz. Notwithstanding these considerable dynamic forces, the chert rock along the walls of these fault fissures is very little disturbed; the same may be observed in the Vermillion district, where a double folding of the formation occurred, but the position of the cherts is still nearly horizontal. Now these waters, in order to reach the cherts would have to pass throught the belt of

weathering, and would have dissolved any iron in their path. The chert, as we already know, consists of a mechanical mixture of carbonate of iron and a cherty silica, consequently, as soon as the water reached this carbonate of iron, the carbonic acid would have precipitated the iron out of such waters, the iron precipitates would have clogged up the interstices and capillaries of the chert, and the water would have been diverted elsewhere, and would never have reached the base of the chert, where we usually see the iron resting. Supposing that water No. 1 really had not dissolved any iron from the belt of weathering, but that it had actually found its way in and through the chert and had extracted the silica, would it not, at the same time, have dissolved the carbonate of iron, and would not both have left the troughs together? If so, then water No. 3 would have found neither carbonic acid nor carbonate of iron for precipitating the iron out of water No. 2, and the whole process would have been impossible that is to say, we would find no iron deposits to-day. But why should only water No. 2 be able to dissolve iron from the weathered products, and why not also water No. 1? Who can solve this riddle? Further, in looking at the sections of the troughs in which the iron deposits rest, we notice that the water had to ascend to a considerable height in order to escape again. If what Mr Van Hise claims is true, the hydrostatic pressure must have been equally considerable, and would it have been possible to deposit anything under such a pressure? And further, water that is standing in a basin or trough does not permit of the dissolution of any kind of rock, even under pressure, and if this rock was submerged, the water could not under the circumstances have dissolved the silica out of the chert.

I must repeat here that these Keewenawan sandstones, marls and conglomerates did not contain sufficient iron to make one decent iron ore deposit. These rocks can be seen here, in our country, under an overflow of trap, still intact, and their iron contents may be easily judged. Besides they were not deposited everywhere, and in many places only in shallow layers, therefore the belt of weathering has played a very subordinate role. Even supposing that there had been iron in considerable quantities in this belt, would not water No. 1 have leached

every particle out of it, considering the length of time it must have taken to dissolve the silica out of the chert, transport it out of the troughs and make room for these large deposits of iron ore? Would not this light, flocculant iron precipitate have suffered the same fate as the silica?

What I have said above will show that Van Hise's theory does not rest on a solid foundation, for the following reasons:

(1.) There was no belt of weathering which could have furnished sufficient iron for these massive deposits.

(2.) Water that percolated through a ferruginous weathered rock material would dissolve the iron by the first contact and not after it had first done other work.

(3.) Water that flows down one slope and has to surmount another fills that space into which it flows, and is under static pressure, actuating a force that would not permit of any undisturbed precipitation and settling of light precipitates.

(4.) Rock under water are not oxidized or markedly changed,

(5.) Water that dissolves hard chert rock also dissolves carbonate of iron mechanically mixed with it, and if the one is washed away the other has to follow.

(6.) There would not have been merely a sagging, (as Mr. Van Hise says) but a caving of the rock formation into the spaces from which the silica was eliminated.

At this point I may also mention that Mr. Spurr attributes the existence of the iron deposits to the oxidation of "Glauconite." I myself have not been able to detect this mineral in any of the cores or mines that I have examined, but there exists abundantly throughout the whole formation a green mineral and a skeleton of siderite, and in some instances also quartz, both surrounded by hematite. This green mineral is the altered product of a mixture of siderite and chert, in the form of granules and surrounded by carbonate of iron, from which the carbonic acid has been expelled, the ferrous oxide saturating and coloring both. The mineral is quite soft and is attacked by the ferric oxide surrounding it. The pure quartz which is sometimes found remaining in these cores has not the same cherty appearance. It has a granular,

subtranslucent aspect, and is also in the course of being changed into hematite.

Dr. Leith, of the United States Geological Survey, is perhaps nearer to the truth regarding the derivation of the iron ore deposits, when he advances the theory that the above mentioned green mineral, which he names "Greenalite," is the one which has furnished the iron. Unfortunately, when I recently had the pleasure of meeting Dr. Leith, the time at our disposal for conversation was only too brief, so that I am not quite fully informed as to how he considers the process to have taken place. I was also unable to procure his monograph on the Mesaba, which otherwise might have given me the desired information. Only this much I gather that he seems to consider this "Greenalite" an ideogenetic mineral, and that from its appearance dates the commencement of the iron formation.

If we take into consideration the fact that the Mesaba iron-bearing rocks and iron deposits are very much truncated, it is readily understood how any observer could easily be misled in regard to this "greenalite" being an original mineral, for the upper, undisturbed and unaltered part of the chert is eroded and swept away there. Here this is not the case, and we are therefore in the fortunate situation of being able to see at many places the whole Animikie rock series still intact, as it was laid down. And from study of this we learn, as I have repeatedly stated above, that the original mineral is a chert and siderite granule, and that the "greenalite" is merely an alteration product of the former. Of course, if we take exception to this, that is to say, if we give no consideration to its original condition and begin to date the origin of the iron from the appearance of this mineral, then perhaps we might be in a measure justified in giving this green mineral a new name. In that case naturally we have to shut our eyes to the fact that this green mineral represents a certain phase already accomplished towards the forming of the iron. There is, however, another side to this question of the origin of the iron, namely that it did not owe its existence exclusively to the "Greenalite," but also to its original mineral ferruginous chert, yes, even to the jasperized chert, as we may see in some localities on this side, where large areas of this material are

changed into hematite. These different processes can be observed at many places. It is, therefore, problematic whether we are justified in assigning to the green mineral granules an importance which they do not quite deserve.

Neither the one nor the other of the above hypotheses fits correctly the origin of the iron deposits; my opinion of their origin, expressed in a few words is as follows:

"Water with free oxygen percolates along the slopes or other places in the ferruginous chert rock, descends through the capillaries of the chert, oxidizes the carbonate of iron, forming ferric oxide, and *the ferric oxide absorbs the silica.*"

I have no doubt that many after reading this will cross themselves in holy horror at such an heretical assertion, but I cannot help this, and as long as they are unable to prove me wrong, I won't go to "Canossa."

Our ever advancing scientific researches reveal to us almost every day something new, shed a new or clearer light on old doctrines and theories, and prove to us that many things which for years and centuries were regarded as practically gospel truths, are after all but erroneous opinions. For instance, they now tell us that these dear, solid, indivisible pets of ours, the elements, are no elements at all; that they are derived from one common source, radiating forth from what and whence they came. They further tell us that these elements—metals—are only weak beings, and not a hairsbreadth better constituted than we poor mortals, that they can become sick, yes, even could be poisoned.

Perhaps it might be of interest to relate here an experience in regard to this which I had several years ago with chemically pure tin-foil. One day, when I was going to make some chloride of tin, I observed that the upper portion of the sheets of tin, which I had in a cigar-box, laid out with paper, was perforated by a number of almost geometrically round holes, the rim of which looked dentated or jagged. My first impression was that I had spilled some hydrochloric acid on the sheets, and at the time I paid no further attention. Some time afterwards, coming across the box again, I saw the whole surface of the upper sheets full of holes, and eaten down through nearly one-half of

the few hundred sheets of foil which were in the box. I took out the unaffected foils and wrapped them in a paper. A short while later I examined these, but could not detect the least affection. I now transferred two of the healthy sheets and placed them with the diseased, to see whether the latter would infect the former or not. In a very short time they showed exactly the same symptoms as the other. I now left this box undisturbed for several months but on returning home after a lengthy absence and taking up some work in my laboratory, I remembered the tin foil, opened the box, and found remaining only a small quantity of a greyish white substance, mixed with a few small specks of metallic tin. This could not have been more than from 5 to 6 grams, from at least 10 to 15 grams of the original tin foil. I put the box back with the intention of examining the contents later. Being absent for several weeks thereafter, I had no opportunity of doing so, but on my return I concluded that I would try to solve the mystery. However, on looking for my box, it had disappeared, and after every nook and corner of my laboratory had been ransacked, I found out at last from my assistant that he had relieved me of this work and used the "empty" box for kindling. What was it that caused this disintegration and contagious disease, what the seeming diminution of volume and weight? Was it the tin pest in its grey modification? If so, and if we attribute the disease to excessive cold, why was not the rest similarly affected, although exposed to the same temperature? It became so only after being in contact with the other.

Now if one has to credit these things, and I see no reason for not doing so, with as good reason as in the case of what we formerly believed to be the truth, then the reader has also to accept what I have claimed above, *i. e.* that there are among the list of our so-called elements "elements of prey." Perhaps the name "element" would no longer be the correct term; according to this new theory, "Electrons or Ions of prey" would be better. Now whether these electrons have a selective preference, according to their respective tastes, I do not know, but of this much I am certain, that there are two which would come under this classification—ferric oxide and magnesia.

For years I have studied and tried to bring the old chemical,

"Replacement Theory" into harmony with what I saw in the field and the mines, and especially in regard to the formation of the iron ores around Lake Superior. I did not succeed, and again and again I was led to the conclusion that some other process must exist for the production of these iron ores. There was no other way, no other possibility than to consider an "Absorption Process" by which these ferruginous cherts were converted into iron ore. Now in our case here, let us consider :

(1.) There is no flowing water, that is water flowing with such force into these ferruginous chert deposits as to carry anything out ; it is simply a moistening action by capillary descension through permeable rock. Further it could carry nothing in because there is no residual material from which anything could be extracted. If it had carried iron in solution into this rock, the carbonic acid of the siderite would have precipitated the iron out of it, and the precipitate would, by filling the capillaries, have stopped the whole process right at the beginning.

(2.) Through the oxidation of this rock and ore, heat is evolved and the incoming water is evaporated as fast as it comes down, and, fulfilling its chemical action, leaves the rock with the carbonic acid, which might also be a militant factor in attacking the silicate acid and preparing the way for the attack of the ferric oxide. The evaporation of water was doubtless considerably greater in the lower depth of our rocks, on account of the greater heat existing therein, especially in that period of recurring eruptions.

(3.) Moisture with free oxygen attacking the carbonate of iron which is mostly mechanically mixed with the cherty silica, is changing the siderite into ferric oxide ; the silica particles softened through the action of the carbonated water are then absorbed by the ferric oxide with the help of a fresh supply of oxygen.

I shall with the assistance of the new atomic theory mentioned above, explain this phenomenon in more detail and from a more practical standpoint, and thereby we may arrive nearer to a possible solution.

We can say with almost certainty "that all matter is electricity," and further that electricity exists in three forms, (1), Static Electricity,

(2), Electricity in motion, and (3), Electricity in accelerated motion. Hence, (1). An unaltered fresh piece of our rock would be a body—let me say—a latent body of electricity.

(2.) When this body becomes animated from one cause or another, we would have the second form, a body of electricity in motion.

(3.) When this motion becomes accelerated through a second cause, then we have the third form.

How can we explain this motion? I shall endeavor to illustrate this in the case of our rock,—the siderite and chert.

I have applied the term motion only to forms (2) and (3), but I must not omit to mention that there is a motion which exists in form (1), although it is only a motion within itself. This motion is that of the electrons which constitute the atoms of that body, of which we are taught that they follow a revolving motion of immense rapidity among themselves. To use an illustration, I might say that it is similar to that of the planets in the solar system. This then is the motion of the first form, which is called a latent motion, because it is not exerting any influence on an outside body. But what constitutes the motion of the second form? Perhaps the first impulse to this motion was the interference with the latent condition of the siderite by an outside medium, by which the carbonate of iron was changed into ferrous and ferric oxide, or, to express it in another way, by which the carbon dioxide of the siderite was eliminated, and oxygen took its place, sometimes in such proportion that the whole of the remaining ferrous oxide was converted into ferric oxide. It is likely that the medium which caused this change was water with free oxygen. The direct sequel of this action of the water and oxygen was another phenomenon, namely the evolution of heat by the oxidation of the siderite and liberation of carbon dioxide. This heat, added to that already existing in those rock strata, evaporated the water, which delivered the free oxygen. The steam produced thereby commingled with the liberated carbon dioxide and both attacked the silica of the chert, softening the chert granules. Thereby, in both these minerals,—iron and silica—was produced an "animated" motion.

Next we come to form (3), "the accelerated motion," for the production of which we may account in the following way :

Owing to the almost constant volcanic activity in the pre-Cambrian period, by which many dykes of different rocks were intruded into the Animikie rock strata, fumaroles and solfataras were produced, whose gases and fumes mingled with the meteoric waters ; these acidulated waters also percolated into the spaces spoken of above, and formed electrolytes for the iron oxide and silica, which, as we have seen, were already in their second form of motion, but then arrived into their third form, "the accelerated motion." Now, as to the possibility of this, let us consider the iron oxides as a positive element,—the anode,—and the silica as the negative element,—the cathode,—and assign to the acidulated water the function of the electrolyte. Some of these FeO and Fe_2O_3 particles dissolved, formed the anions, which would rush over to the cations of the cathode, and either combined with them to form Dr. Leith's "Greenalite," or took their places altogether, absorbed, them and formed ferrous, and at last ferric oxide electrons. In our study of these phenomena we might further take into consideration that the earth's magnetic or electric currents, which in the earliest history of our globe must have been considerably stronger and more frequent than at present, played doubtless a conspicuous part, in these changes in rocks and minerals, and it is therefore highly probable that they did so in the case we are dealing with.

Whether such a process as that described has existed for the conversion of the ferruginous cherts into iron ore, must of course be only a matter of speculation ; but after studying the whole conditions under which this rock occurs, and how the changes within it took place, one can reach no other conclusion than that there was not, nay, there never could have been a replacement process in play, dissolving and eliminating the silica from this rock, and making room for the deposition of ferric oxide in its place. I must therefore again express my belief that there have been and are still phenomena going on in our earth's crust which are clouded in darkness, but with a better understanding and development of the new atomic theory, it may be that we will arrive also at a better knowledge of the methods by which these changes in

rocks and minerals and ore deposits take place. The above hypotheses are simply brought forward by me to give a possible explanation of what occurred in these rocks, and of the formation of these iron deposits, comparing them with processes which we have before us every day, and supporting them with phenomena which have of late produced so profound an impression on every observer.

Some of the readers may ask, whence these deductions? I reply, from my studies in the field, at the mine and in the laboratory.

(1.) We have in this country a number of places where, on precipices, very plain sections of rock and iron are exposed, and where the process of converting the chert into iron ore was suddenly interrupted by the heat of the trap lava which overflowed the rock. Here we can see in the clearest manner possible how the iron ore was formed. Through the heat of the trap lava, hematite or limonite was converted into magnetite and the process ceased. The iron rests, I might say alternately, between the laminae of chert, in varying thickness, but seldom over one foot, between layers of chert equally thick. In no instances can there be seen hollow spaces in the chert, not even honey-combed pieces, which might be recognized as indicating a removal of silica. Iron and rock are merging imperceptibly into each other, as if they were originally laid down together. Above and below these iron layers, we see how the iron was eating its way into the chert; sometimes two or three layers will connect at one or the other side or end. Now this iron, which is usually of very high grade, (68% to even 70% not being rare), is not of great extent horizontally and can be seen resting in the chert, by which it is often surrounded on all sides. No channels are visible by which the surface water could have found its way in, except by capillary descension or through percolation along the interstices of the laminae. This latter is however rarely observed, because in that case the whole infiltration channel would show signs of oxidation. The cherts lie horizontal, hardly any dip being observable. If the replacement theory were correct, open spaces, small or large, should be seen; also an infiltration channel through which the iron had come down and been precipitated into these spaces from which the silica had been removed. The reason why the iron lies nearly alter-

nately between the chert may be the unequal oxidability of the rock. For, as I have remarked above, the iron carbonate in the chert varies in amount, which may be attributed, besides to the causes before mentioned, to the difference in the specific gravity of the two minerals.

(2.) In examining the hard ores in the workings of the Vermillion mines, we frequently notice how the pure iron is merging into the chert; even in the pure ore layers we find specimens which show no external difference, either in structure or color, from the chert, until, on breaking them open we find the pure iron. The gradation from the good to the poor iron is often so imperceptible that it needs a trained eye to detect it. If the iron had been carried in there, we should see principally good, pure iron, and not iron in all stages of purity, or—to express it differently—cherts in all stages of conversion. This can be observed on all ranges.

(3.) This process of the conversion of the cherts may be easily observed in operation upon a number of outcrops which were for long periods under water, and now through the receding of Lake Superior are exposed on the surface. Oxidation is progressing constantly by exposure as well as under cover of humus. Where the deposit is covered by humus and sod, the process goes on uninterruptedly, and it consequently shows a considerably greater advance because it is always kept moist and nothing is carried in except water and oxygen. Here we have another instance of iron being formed without being brought in from a belt of weathering.

(4.) It is principally by examination and study of drill cores from a large number of different places that we are enabled to observe and learn the various phases of this conversion or absorption process, but I do not remember having seen one from which we could draw the conclusion of the probability, or even the possibility of a replacement process. We see perhaps here and there at the surface of the chert rock specimens which have a honeycombed or amygdaloidal structure, but in this case it is readily seen that the oxidation products were washed out by the rain water.

(5.) In proving the existence of an absorption process, we do not need to confine ourselves to a single locality. I have seen cases of

whole white clay beds, resting in spaces of an impermeable granite whence no means of escape for the silica or alumina existed, where ferric oxide formed all shades of yellow and brown ochre, even to a hard limonite. The same occurrence I noticed in the case of a white marl. Observe what Pliny says about the "Eagle Stone." (Aetites, Plinius, 36, 39.) Among other things he writes, "In the interior, (literally abdomen), they have a hard stone or soft clay which rattles when shaken." I have seen them almost solid, in which the interior, formerly silica, clay or lime, was completely changed into iron. I have also seen others which were solid, and had gathered about the periphery one layer after another of clay or other material, forming thereby concentric layers which consisted of nothing beyond the aforesaid materials, the outer of these layers being in all stages of the metamorphosis into iron, as could be easily observed when they were dug out of their resting places. Where did the replacement products go to, there being hardly any seepage of water to the outside? With the well known "bean ore" the process is the same. Possibly these phenomena are accompanied by some other reactions by which the whole process is assisted or accelerated, for instance, through carbonated or acidulated water, as I suggested above.

(6.) I mentioned above that magnesia acted in the same manner as ferric oxide. As an example I would give the peridotites of Maryland and Carolina, forming intrusions in a gneiss or granite. The final product is a beautiful white talc, no signs being visible of a replacement or dissolution of other minerals, with the exception of nickel, in the form of a genthite in the seams of the serpentine. This mineral, however, also disappears as the process advances farther.

Most interesting is it to follow this process in some of the Maryland rocks. At one place we have the unaltered peridotite, a little farther on we see the same rock changing into serpentine, still farther we notice a fine dark green serpentine, next we observe amygdulæ eaten into it by the oxidation of the ferrous oxide into ferric oxide; this process must have been interrupted, the holes washed out and prehnite and sodalite infiltrated into them. Next in order we see this rock changing into talc, the ferrous oxide disappears, it turns white, but the

shape, form and volume of the rock remains, with amygdules and their contents intact, only these also changed into talc. Now I ask where did all the other minerals go to? Were they exchanged, molecule for molecule, for a silicate of magnesia? Whence then came all this magnesia? Not from the olivine alone. There are no signs of an influx of water containing magnesia any more than is the case with any other basic rocks changing into serpentine.

We could multiply the cases wherewith to prove that certain elements have the power of absorbing others, that is, their bases in ions or elections are stronger and absorb the weaker, and it may be that this theory will harmonize with the new theory of the origin, animation and existence of the elements, and it may be further that we will discover that most of the so called "metasomatic" changes in rocks and minerals are nothing other than "absorption processes," such as that to which I ascribe the origin of the iron ore deposits around Lake Superior.
